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PAGE

E.S.P. PERFORMANCE AND THE EXPANSION-COMPRESSION RATING BY D. J. WEST . . . . .	295
THE SHACKLETON EXPERIMENTS: A COMMENT BY S. G. SOAL ON AN INVESTIGATION BY R. C. READ . . . . .	309
VISIT OF DR J. B. RHINE . . . . .	310
REVIEWS . . . . .	311
CORRESPONDENCE. . . . .	315
OBITUARY . . . . .	319

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# JOURNAL

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VOLUME XXXV No. 660 SEPTEMBER-OCTOBER 1950

## E.S.P. PERFORMANCE AND THE EXPANSION-COMPRESSION RATING

BY D. J. WEST

### I. THE USE OF THE EXPANSION-COMPRESSION RATING IN SEPARATING POSITIVE AND NEGATIVE SCORERS

THE possibility that successful E.S.P. subjects might be characterized by special personality features has stimulated some interesting research in the United States. Dr Betty Humphrey, of the Parapsychology Laboratory at Duke University, setting out to look for a personality test that would enable an experimenter to select subjects likely to produce good scores, came across Dr Paula Elksch's method of classifying people as *expansive* or *compressive* (3). In several series of experiments (4, 5, 9), Dr Humphrey found that subjects rated as expansive by the Elksch method tended to produce positive scores (i.e. above chance expectation) in clairvoyance tests. In G.E.S.P. tests,<sup>1</sup> however, the situation was reversed, the compressives tending to score above chance and the expansives below.

The E.S.P. data used in these tests were drawings made by subjects in an attempt to reproduce a concealed target picture. The correspondences between subjects' drawings and target drawings were evaluated by C. E. Stuart's preferential matching technique (14, 15, 16). The same drawings as had been obtained from the subjects in the E.S.P. tests were used for the psychological rating. Dr Humphrey did not know the E.S.P. scores at the time when she allotted the expansion-compression ratings.

Each subject produced a set of four drawings aimed at four different targets. In the preferential matching method two independent judges both match all four drawings of each subject

<sup>1</sup> G.E.S.P. (General Extrasensory Perception) tests are those in which an Agent looks at the target. In other words, the conditions are such that either telepathy or clairvoyance might be operating.

against all four targets, and then all four targets against all four drawings. The targets are given to the judge in random order and he is kept in ignorance of their true arrangement until after he has finished scoring. Each time the judge matches a drawing, he puts a 4 against the target he thinks it matches best, a 3 against the target it matches second best, and so on. Some such table as the following is produced :

DRAWINGS	TARGETS			
	I	II	III	IV
I	4	2	3	1
II	1	3	2	4
III	2	3	4	1
IV	1	2	4	3

E.S.P. score 14

The sum of the scores in the leading diagonal is taken as the E.S.P. score. Chance expectation is 10. Since the two judges together produce four such tables, the total chance expectation is 40. The following table summarizes Dr Humphrey's published results of the expansion-compression analyses of drawings :

Reference No. of Experi- mental Report	Nature of E.S.P. test	EXPANSIVES		COMPRESSIVES		<i>t</i> (differ- ence)	<i>P</i>
		No. of Sub- jects	Mean Score <sup>1</sup>	No. of Sub- jects	Mean Score <sup>1</sup>		
(2)	Clairvoyance	41	41.88	55	37.45	3.00	.0026
(7)	Clairvoyance	36	43.28	59	38.27	3.51	.00023
(3)	G.E.S.P.	105	38.23	134	40.96	2.95	.0032

<sup>1</sup> Chance expectation = 40.00

This table includes six separate series of clairvoyance data, in all of which the direction of the difference in score between the expansive and compressive groups was the same. Similarly, in six subdivisions of the G.E.S.P. data the direction of the difference was consistent. In all three reports the *total* E.S.P. score did not differ significantly from chance expectation, but after the subdivision into expansive and compressive groups it became apparent that the null result might have been due to mutually antagonistic trends in different subjects. The hypothesis suggested by the results is that in an unselected group of subjects some score positively, while others have a tendency to avoid the target and so



score below chance, and that these scoring habits bear some relation to the subjects' expansive or compressive trends.

Other experimenters have reported experiments using the expansion-compression rating with E.S.P. drawings. J. M. Bevan obtained results showing trends similar to those described by Dr Humphrey (1, 2). Some clairvoyance drawings done incidentally to PK tests by Nash and Richards gave insignificant results (12).

In co-operation with other experimenters, Dr Humphrey also investigated the relation of expansion-compression rating to scores by the same subjects on clairvoyance tests with cards (7, 9, 10). The total results were as follows :

CLAIRVOYANCE CARD TESTS

EXPANSIVES		COMPRESSIVES		C.R. (difference)
No. of runs	Deviation	No. of runs	Deviation	
575	+58	926	-69	1.13

The table shows that no significant total result was obtained in spite of the large number of runs. In a relatively short series of pure telepathy tests with card symbols, E. A. McMahan obtained a significant difference between the scores of expansive and compressive groups (11) :

PURE TELEPATHY TESTS

EXPANSIVES		COMPRESSIVES		C.R. (difference)
No. of runs	Deviation	No. of runs	Deviation	
36	-15	60	+40	2.57

This result is of special interest because the same subjects had previously been used in clairvoyance tests in which they had shown trends in the opposite direction to those obtained in these pure telepathy tests. Thus the reversal of scoring trends between clairvoyance and telepathy conditions, observed by Dr Humphrey, here receives some confirmation. On the whole, however, the expansion-compression rating has been found of reliable application to card tests only when in combination with some other personality rating, in particular the Stuart Interest Inventory (17).

## 2. THE NATURE OF EXPANSION AND COMPRESSION

The discovery of a measurement which predicts the likely direction of a subject's score in E.S.P. tests is no more than a

preliminary step to further investigation. One needs to know something about the qualities of expansiveness and compressiveness, and to find out how and why they are connected with E.S.P. performance.

The criteria used by Dr Humphrey are based upon a projective test devised by Dr Paula Elksich. In her investigation Dr Elksich utilised the form quality of children's drawings, certain features of which she considered to be indicative of neurotic trends. The criterion of expansion in Dr Elksich's test 'stands for a direction toward the surrounding world ; for the potential ability of making contact' (1) and may indicate freedom, courage, adventure, and a healthily developed extraversion. The criterion of compression, however, 'stands for isolation' and may indicate neurotically developed introversion.

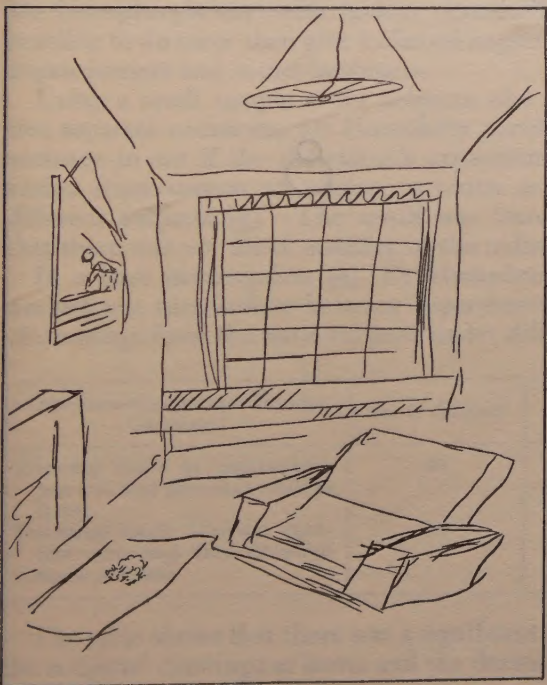
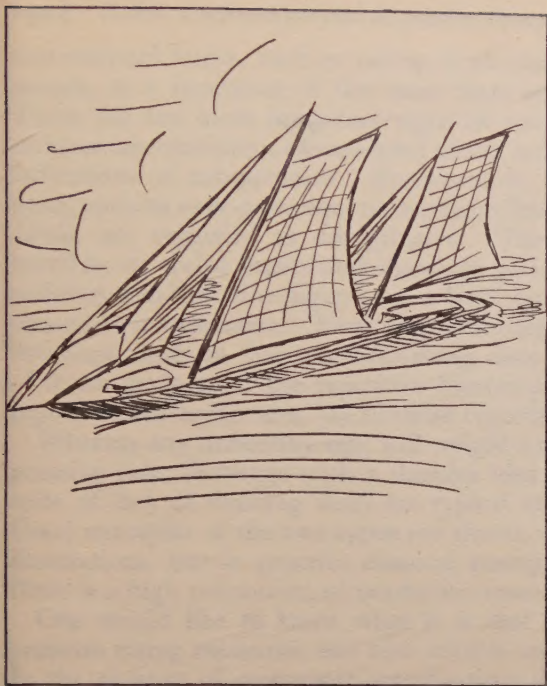
As used by Dr Humphrey, the rating scale of expansion-compression and the criteria on which it is based differ slightly from the original Elksich version. The following description is based upon information given verbally by Dr Humphrey in October 1949, when she instructed me in her method of rating drawings.

Each drawing is given a score of 0,  $\frac{1}{2}$ , or 1. A score of 1 means no definite indication of compression ; a score of  $\frac{1}{2}$  is given when the judge finds it impossible to say whether the form quality is expansive or compressive ; a score of 0 means definite signs of compression. Each subject makes four drawings, so that his total score can vary in units of  $\frac{1}{2}$  from 0 to 4. Subjects with scores of  $1\frac{1}{2}$  or less are classed as compressive ; those with 2 or over as expansive.

In rating particular drawings Dr Humphrey allows herself to be guided by the general trend of all four of the subject's drawings. The best judgements, in her opinion, are given by quick, comprehensive appraisals rather than by a painstaking dissection of individual features. This is perhaps analagous to Allport's observations on the falling-off of efficiency in the interpretation of facial expressions when the judges became too consciously analytical.

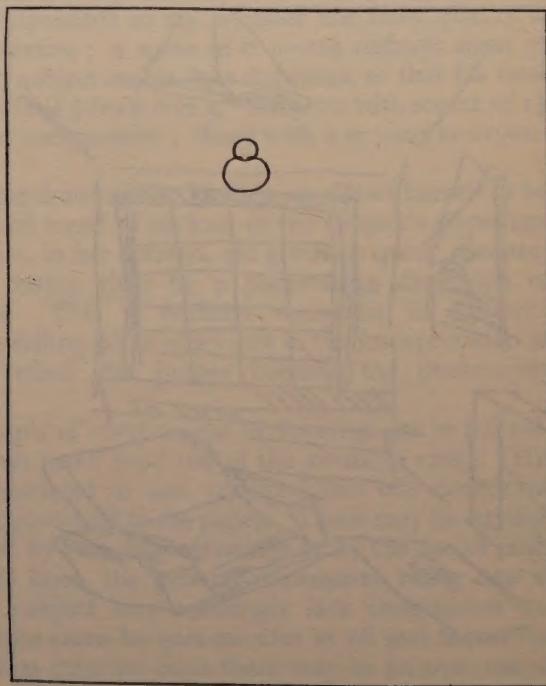
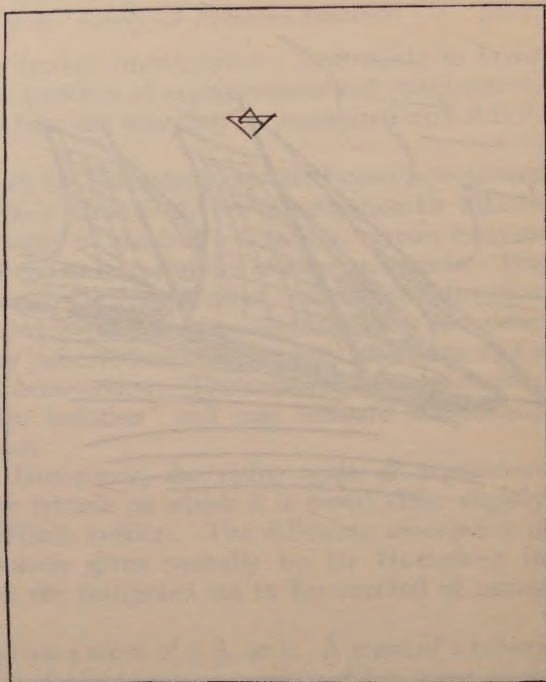
The significant signs of compression in drawings are as follows. The subject does not make good use of the available space. His drawing may be restricted in size, cramped into one corner, or otherwise badly proportioned to the paper. There may be signs of timidity, manifested by very faint drawings or by the use of jerky or feathered pencil lines, the general impression being one of uncertainty. The subject may seemingly lack imaginative expression. In extreme cases he gets no idea at all and leaves the page a blank. In less extreme cases there may be an over-use of





These drawings are from one of my expansive subjects. The bold use of available space, the free, vigorous style, and some imagination in the choice and representation of topic are seen in both drawings.

These two drawings, obtained in the course of my experiment, were drawn by a compressive subject. The poor use of space and the lack of freedom and imagination are well shown.





conventional forms, such as sailing boats and 'stick' figures for people, or a repetition of the same form in different drawings. These are the most important signs of compression, but other features are sometimes encountered which are included among the indications of compression. For example, obsessive characteristics, such as over-precision, unnecessary labelling, or meticulous detail, are regarded as compressive. The choice of bizarre, horrible, or weird topics and drawings giving an impression of isolation (such as tiny figures on a large background space) are classed as compressive. Features commonly found in psychotic drawings, such as the over-flourishing disorganized productions of manic patients or the repetitive bizarre patterns and symbols typical of schizophrenics, are likewise regarded as compressive.

Whereas any unhealthy sign will weight a drawing on the compressive side, drawings with a definite idea boldly executed (in spite of lack of drawing skill) are typical of expansive subjects. Good examples of the two types are shown in the accompanying illustrations, but in practice classical examples are unusual and there is a high proportion of borderline cases.

One would like to know what it is that the expansion-compression rating measures, and how reliable are the measurements. In the absence of systematic investigations of the correlations of Dr Humphrey's test with known personality factors, it is impossible to do more than give a clinical impression of the nature of expansiveness and compressiveness.

Using a small sample of six subjects who were each tested on two separate occasions, Dr Humphrey performed an analysis of variance to see if the individual's expansion-compression rating varied from session to session as much as did the ratings of different subjects (5). The result was inconclusive, suggesting that there was not much stability in the individual rating.

In a later investigation (9), Dr Humphrey and C. E. Stuart worked out correlations between expansion-compression ratings of drawings from the same subjects under different conditions :

<i>Expansion-Compression Ratings Correlated</i>	<i>No. of Subjects</i>	<i>Correlation Coefficient</i>
Drawing made in clairvoyance test and free drawings made at home	24	$r = +0.4745$ ( $P = 0.018$ )
Drawings made between card- test runs and free drawings made at home	25	$r = +0.0506$ (Insignificant)

The table shows that there was a significant correlation between the subjects' drawings at home and the drawings which they pro-

duced in the course of a clairvoyance test. There was no correlation between the drawings done at home and the drawings done in the limited time available between runs of card-calling tests. In the latter case the centre of interest was probably not the drawing.

Dr Humphrey noticed that the proportion of expansives and compressives, usually about equal in her own groups, varied with other experimenters. With conditions that were uncomfortable a high proportion of compressive drawings was obtained.

In the course of my own experiments I became convinced that there were certain types of subject who would inevitably produce highly compressive drawings. These were the people who sat nervously on the edge of their chairs, excused themselves repeatedly for their inability to draw, looked extremely uncomfortable when they tried to do so, and took a long time to get anything at all down on paper. Compressives are probably people who tend to be shy and uneasy in the test situation. Whereas most people might be induced to draw compressively if placed in sufficiently intimidating circumstances, habitually inhibited and introverted characters would probably produce compressive drawings whatever the conditions. There seems no doubt that marked compression, as defined by Dr Humphrey, is an unhealthy feature. On the other hand, the slight and transient variations in expansion-compression rating which appear to be sufficient to change the direction of a subject's E.S.P. scoring are clearly without clinical significance.

It would seem that the expansion-compression rating probably depends upon personality features which are unstable in that they are influenced by the attitude and mood of the subject and the conditions of the test. Only when a subject shows expansiveness or compressiveness in an extreme degree is he likely invariably to give the same rating.

One could take advantage of the instability of the expansion-compression rating to test whether there is a shift in E.S.P. score when the subject changes from expansive to compressive mood. Dr Humphrey tried this on a small scale with suggestive results (5, p. 190).

Whatever the expansion-compression rating may measure, there is considerable evidence that it is something more than Dr Humphrey's own arbitrary and subjective opinion. In independent ratings of the same data by Dr Humphrey and C. E. Stuart, correlation coefficients ranging from  $+0.50$  to  $+0.75$  were obtained from different series of drawings (2, 8). Independent ratings by Mrs Dorothy Pope of some of Dr Humphrey's data (2) were correlated to a lesser degree ( $r = +0.40$  for 96 sets of draw-



ings). In fifty sets of drawings collected by me in this country, rated, and then sent to Dr Humphrey for an independent judgment, the correlation coefficient between the two was +0.53. Considering the limited amount of instruction I received and the absence of opportunity for subsequent consultation, this result compares favourably with the Duke figures.

Although Dr Humphrey remarked of the sample of British drawings that they were 'practically indistinguishable from those of college students here', it was noteworthy that there was an unusually high proportion of compressives:

DISTRIBUTION OF SCORES AMONG DR HUMPHREY'S RATINGS  
OF 50 SETS OF DRAWINGS BY BRITISH SUBJECTS

Size of Score	-	0	$\frac{1}{2}$	1	$1\frac{1}{2}$	2	$2\frac{1}{2}$	3	$3\frac{1}{2}$	4
Frequency of Score	-	15	10	6	8	7	1	3	0	0
39 compressives					11 expansives					

This result could have been due to a difference in the behaviour of the British and Americans, to an atypical trend in the British sample which contained a high proportion of the experimenter's friends, to the effect of the experimenter's personality and method of approach upon his subjects, or to the conditions of the experiment.

### 3. DESCRIPTION OF AN EXPERIMENT

During the period November 1949 to February 1950 I carried out an experiment utilizing the expansion-compression criterion in E.S.P. tests with fifty British subjects. The subjects were tested one by one at fifty separate experimental sessions. It was hoped that individual attention might be more conducive to successful scoring than group tests, since this method enabled personal contact to be made with each subject. Sometimes the sessions were combined with a social occasion, sometimes refreshments were provided, and in all cases an effort was made to produce the comfortable, friendly atmosphere that is said to be the preliminary to good results.

Another reason for taking the subjects individually was to avoid certain difficulties inherent in group tests. For example, multiple calling of the same target pack, or interdependence of drawings of different subjects (due to such factors as subjects copying from each other) might possibly give rise to spurious effects.<sup>1</sup>

<sup>1</sup> A discussion of some of these difficulties can be found in *J. Parapsychol.*, 1947, **11**, 133-6.

Most of the American work has been done with young college students as subjects. In this experiment nearly all the subjects were in their twenties, only one or two being over thirty years old. Volunteers from the experimenter's lecture audiences and friends were the sources from which subjects were drawn.

The experimental sessions were conducted without witnesses. At each session the subject was asked to do four runs of cards and four drawings. He was first presented with a drawing pad and was told by the experimenter that underneath this there was a picture. He was asked not to make any special effort to concentrate on the concealed picture, but to draw whatever first came to mind. After two drawings had been completed, two card-calling runs were performed, then another two drawings, and finally the third and fourth runs of cards. The target pictures, which were different for each subject, were known to the experimenter, so that this part of the experiment was under G.E.S.P. conditions.

There were prepared and recorded in advance of each session by an assistant<sup>1</sup> four packs of randomized E.S.P. cards. In the card-calling tests the experimenter went into an adjoining room, leaving the door ajar, and sat behind a small screen. The subject was asked to call aloud twenty-five guesses, writing them down on a score sheet as he went along. He was told that the experimenter, after giving the signal to begin calling, would turn over the cards one by one without speaking, but keeping in time with the subject's guesses. At the end of the run of twenty-five calls the experimenter came back into the room with the target pack, which was then checked against the guesses in the subject's presence. The guesses were later re-checked against the record of the target pack previously prepared by the assistant.

The object of using prepared packs was twofold. No loophole was left for recording errors on the part of the experimenter. It was possible to carry out alternate runs of card calls under G.E.S.P. and clairvoyant conditions. In the latter test, the experimenter, without telling the subject,<sup>2</sup> did not look at the target pack until after the calls had been made.

The subjects' drawings were sent to Dr Humphrey who rated each subject on the expansion-compression scale. The target pictures were randomized, code-numbered, and then dispatched to Duke University where Dr Humphrey and Miss Elizabeth McMahan acted as the two judges in a preferential matching evaluation.

<sup>1</sup> Special thanks are due to Mrs Gale Thomas for her help in this connexion.

<sup>2</sup> In this respect the test differed from the clairvoyance experiments at Duke in which the subjects always knew what the conditions were.



## 4. RESULTS AND DISCUSSION

In the G.E.S.P. test with drawings no significant deviation from chance expectation was obtained, nor was there any significant difference between the expansive and compressive groups of subjects.

EXPANSIVES		COMPRESSIVES	
<i>No. of Subjects</i>	<i>Mean Score</i>	<i>No. of Subjects</i>	<i>Mean Score</i>
11	39.18 (Chance expectation = 40.00)	39	39.07 (Chance expectation = 40.00)

Dr Rhine has commented that the failure of this test to show any E.S.P. effect may have been due to the subjects' attention not being specifically directed to the target. Had the pictures been in the same place as the cards, and had the same emphasis been placed on getting the pictures correct as on getting the cards right, a different result might have been produced. It must be admitted that the experimenter's interest was centred upon the E.S.P. card tests.

In the card tests the total deviation was in a negative direction and not statistically significant. The G.E.S.P. tests considered independently were suggestive, and there was a suggestive difference in the subjects' scores on the G.E.S.P. and clairvoyance tests :

	<i>G.E.S.P.</i>	<i>Clairvoyance</i>	<i>Total</i>
No. of trials	2,500	2,500	5,000
Deviation	-43.00	-4.00	-47.00
Critical Ratio	-2.15	-0.20	-1.66

$t$  (difference for G.E.S.P. against Clairvoyance) = -1.60.

When the material was broken up into expansive and compressive groups no evidence of a difference in scoring level between the two was observed, except perhaps for a slightly suggestive difference in the clairvoyance section of the data :

	<i>G.E.S.P. Test</i>	<i>Clairvoyance Test</i>	<i>Total</i>
11 Expansive Subjects	-7 (550 trials)	-18 (550 trials)	-25
39 Compressive Subjects	-36 (1950 trials)	+14 (1950 trials)	-22
C.R. difference	insignificant	-2.07	insignificant

Mr J. Fraser Nicol has pointed out that a neater method for investigating the factors responsible for variation in scoring level is the analysis of variance. He has very kindly performed the following analysis :

## ANALYSIS OF VARIANCE

Source of Variation	D.F.	Sum of Squares	Mean Squares	F (Ratio of Variance to Error Variance)	P
Between Psychological Types	1	12.52	12.52	1.92	~.20
Between G.E.S.P. and Clairvoyance Tests	1	15.21	15.21	2.33	~.15
Type-Test Interaction	1	22.34	22.34	3.42	~.08
Within Sub-classes	96	626.83	6.53		

This result indicates that in the present experiment neither the factor of G.E.S.P. *versus* clairvoyant conditions, nor the factor of expansive *versus* compressive subjects was responsible for any significant variation in score. The interaction figure, however, does give a faint suggestion that, as predicted by Dr Humphrey, one personality type scores better on G.E.S.P., the other on clairvoyance.

The comparison of the difference in total scores of the expansive and compressive groups of subjects is not an appropriate method of evaluation where the hypothesis to be tested is that expansive and compressive subjects tend to score in opposite directions. On this hypothesis, only the direction of the score (positive or negative) is relevant, and the magnitude of the scores should not feature in the evaluation. One way to test this hypothesis would be to prepare  $2 \times 2$  tables as follows :

## G.E.S.P. RESULTS

	No. of subjects scoring below chance	No. of subjects scoring at chance level or above
Expansive Subjects	6	5
Compressive Subjects	24	15

## CLAIRVOYANCE RESULTS

	No. of subjects scoring below chance	No. of subjects scoring at chance level or above
Expansive Subjects	7	4
Compressive Subjects	16	23



## G.E.S.P. AND CLAIRVOYANCE COMBINED

	<i>No. of subjects scoring below chance</i>	<i>No. of subjects scoring at chance level or above</i>
Expansive Subjects	9	2
Compressive Subjects	22	17

Neither in the clairvoyance and G.E.S.P. data taken separately, nor in the combined data, do these tables show a significant difference in the proportion of subjects scoring below chance in the expansive and compressive groups.

It is worth testing another hypothesis, namely that the magnitude of a subject's E.S.P. score is related to his expansion-compression rating. In the G.E.S.P. data a correlation of raw E.S.P. card scores and expansion-compression ratings gave a correlation coefficient of  $-0.07$ , which is completely insignificant. For the clairvoyance data, however, the correlation coefficient was  $-0.249$ ,  $P=0.08$ . Had this result been of more significance, it would have been necessary to make a correction for the non-normal distribution of the expansion-compression ratings.

It would be of great interest to know whether, in the case of Dr Humphrey's data, the expansion-compression rating is related to the size as well as to the direction of the E.S.P. score.

To summarize the results of this experiment, the E.S.P. test with drawings gave a null result, although most of Dr Humphrey's successes have been with drawing tests. There was faint evidence of an E.S.P. effect in the G.E.S.P. card tests, and only faintly suggestive evidence that the expansion-compression rating may have been related to performance in the card tests. However, the sample was small, and it would seem well worth while for another experimenter to try something similar on a larger scale. In any such repetition, however, it would be necessary to pay more attention to the design of the drawing test for E.S.P.

A few subsidiary matters need to be mentioned. The card data were analysed for decline, position effects, and displacement without anything significant being observed. The results are preserved in the Society's files and are available for inspection. Two subjects who had achieved relatively high scores in their card tests were followed up in subsequent card-calling sessions, but in neither case did the subject prove to be a consistently high scorer. In a recent report by Dr Gertrude Schmeidler (13), some results were described suggesting a difference in performance of subjects who were personal friends of the experimenter. In this experi-

ment about half the subjects were friends and half strangers, but when the data were divided into these two categories there was no significant difference in scoring level between them.

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## THE SHACKLETON EXPERIMENTS

A COMMENT BY S. G. SOAL

ON AN INVESTIGATION BY R. C. READ

MR R. C. READ, of St Catharine's College, Cambridge, reports that he has made a careful and systematic examination<sup>1</sup> of the lists of card presentations and guesses in the Shackleton experiments<sup>2</sup> from the actual records in the possession of the Society. His object was 'to see if, by a careful and systematic re-scrutiny of the actual lists of cards called and guessed, it would be possible to discover any clue, such as a significant regularity or irregularity, which might lead to an explanation, other than by supernormal means, of the significant scores obtained, assuming that such an explanation exists'. A suggestion by no means new to us had been made by Mr C. E. M. Hansel that if Mr Shackleton and the Agent were in collusion the latter, if not properly supervised, might during the experiment alter the position of the five cards in the box. If, for instance, the Agent (A) and the Percipient (P) had agreed beforehand that 'precognitive hits' were to occur in positions 9, 14, 19, 24 of the scoring sheet they might arrange for P to call, say, G, L, Z, P in these positions. Having ascertained the order of the five cards in the box during the first few trials, the Agent would at call No. 10 shift the symbol G into the position indicated by the random number presented by the Experimenter controlling the Agent (EA) at the hole in the screen. At call No. 15 he would shift the card bearing L into the position corresponding to the current random number, and so on. This method, if not skilfully carried out, would lead to a piling up of 'hits' in certain positions of the scoring sheet and probably a deficiency of hits in the first five places. As a result of his prolonged investigation, Mr Read has failed totally to discover any evidence that such a method was actually employed. He writes: 'Nothing was discovered which was different from what one would expect, with the exception, of course, of the final result, viz. that (P) scored significantly highly on the card one ahead, and nothing was found in the nature of a clue to a normal explanation of the results obtained.'

Actually, I have no hesitation in asserting that in many of the successful experiments at which an observer watched the Agent the method would not have been possible. Want of space prevented Mrs Goldney and me from publishing in our report every

<sup>1</sup>Space does not permit the publication of this report in full. It may, however, be consulted at the Society's rooms.—ED.

<sup>2</sup>S. G. Soal and K. M. Goldney, 'Experiments in Precognitive Telepathy', *Proc. S.P.R.*, vol. 47, pt. 167 (1943) pp. 21-150.

minute detail of every experiment. But if, for instance, Mr Read will turn to p. 127 of our report he will read that Miss Ina Jephson sat at the experimenter's table during sheets 1-3 and shuffled the cards before each 50 calls *out of sight of both A and EA*. On p. 83 she writes : ' I am also satisfied that Miss E. turned up the correct card indicated by the printed random number held at the little opening by S. G. S. (acting as EA).' In fact, the Agent and EA (S. G. S.) moved to a different part of the room while Miss Jephson shuffled the cards so that there was no opportunity whatever either *before* or *during* the experiment for the Agent to alter their order. The (+ 1) score was equivalent to 3.74 standard deviations.

Our ' Chronicle ' shows that Mr Chibbett, for instance, on 14 February 1941 took identical precautions. As the sole function of the observer when he watched A was to satisfy himself that she turned up the correct cards, he sat, of course, where he could see the cards.

Further, the method suggested by Mr Read and Mr Hansel does not satisfactorily explain (i) the sudden and unexpected appearance of (+2) precognitive hits when the rate of calling was doubled without any previous warning to either P or A or (ii) the phenomenon of multiple determination which a recent examination of the Shackleton records by Dr J. G. Pratt and Professor M. S. Bartlett now makes very probable.

## VISIT OF DR J. B. RHINE

DR J. B. RHINE, Director of the Parapsychology Laboratory, Duke University, delivered the Tenth Frederic W. H. Myers Memorial Lecture at the Caxton Hall on 10 May before an audience of 600. So great was the demand for tickets that all had been disposed of ten days before the lecture, and over 200 members of the public were unable to gain admittance. The lecture, whose subject was ' Telepathy and Human Personality ', will be published towards the end of the year.

On 16 May the Society held an afternoon Reception at the English-Speaking Union, at which many Members were able to meet Dr and Mrs Rhine. In the evening they were the Council's guests of honour at a dinner at the same address.

The reception accorded to Dr Rhine in Europe is evidence of a quickening of interest in parapsychology. During a period of just over two months, his engagements included lectures or informal

talks to fourteen university audiences—at Birmingham, Cambridge (3), Copenhagen, Glasgow, Innsbruck, Leeds, London, Lund, Manchester, Oxford (2), and Utrecht—and eleven meetings of societies concerned with psychical research, psychology, medicine, etc. He also broadcast in the Third Programme and in the Television Service of the B.B.C. His Third Programme talk was published in *The Listener* of 1 June.

## REVIEWS

RICHARD HODGSON : THE STORY OF A PSYCHICAL RESEARCHER AND HIS TIMES. By A. T. Baird. London, Psychic Press, 1949. xxxi, 310 pp. 12s. 6d.

It is forty-five years now since Richard Hodgson died, and, as Mr Baird remarks, it might have been thought likely that his biography would be written sooner, in view of his devotion to psychical research and the importance of his work. One factor which may have contributed to the omission is that Hodgson, an Australian by birth, who spent the early years of his manhood in England, had lived and worked in America from 1887 to the time of his death in 1905, and few people would have had easily available the knowledge required to cover both the English and American periods of his life. However that may be, the present volume will be welcome to many readers to whom it will bring an opportunity of learning what manner of man Hodgson was.

As a result of Hodgson's long residence in America there are few people now in this country who knew him. I myself knew him only when I was a child, but he has left a vivid impression on my mind of the time when he came to England for the sittings with Eusapia Palladino. As Professor William James's son testifies (p. 182), he was a companion in whom children delighted, and to all his friends a most lovable man. Mr Baird has gathered from various sources, especially from Hodgson's fellow-members of the Tavern Club in Boston, evidence of the affection in which he was held.

During his earlier years Hodgson's work in psychical research was largely critical and destructive, beginning with the famous Blavatsky report (Ch. 2), and followed by important reports on the possibility of malobservation of physical phenomena (Ch. 3). During one of his later visits to England in 1895 Hodgson took part in the Cambridge sittings with Eusapia Palladino, and was



instrumental in detecting the trick by which some at least of her phenomena were produced. To the end of his life he remained a confirmed sceptic as regards physical phenomena. His critical bias and his readiness to assume trickery sometimes led him into prejudice, as Mr Baird points out in his account of Hodgson's report on Mrs Thompson, whom various competent observers considered to be a genuine and remarkable medium.

The most important part of Hodgson's work was his long investigation of Mrs Piper's trance-phenomena from 1887 to 1905. It was a fortunate circumstance that this very remarkable medium should have fallen at an early stage of her career into such good and understanding hands : it might so easily have happened otherwise. In Chapters 4-8 Mr Baird gives an account of Hodgson's work with Mrs Piper and the gradual approach to his final conviction that in Mrs Piper's phenomena 'we have empirical evidence of the survival of the dead, and their power under certain circumstances to communicate with the living'.

Ch. 12 of the book deals with the case of Michael Conley, and Ch. 17 with J. H. Hyslop's Piper sittings, and in Ch. 22 there is a short account of the part played by Hodgson as a communicator in the cross-correspondences, and the 'Faunus' incident, reported by Sir Oliver Lodge. On the more personal side Hodgson cannot be considered a successful communicator. When Mrs Piper came to England some years after Hodgson's death, I was given some sittings with her, on the supposition that my own recollections of Hodgson might lead to some evidential result. But to the best of my knowledge and recollection the experiment was a failure.

It is unfortunate that the book is marred by many inaccuracies, e.g. wrong dates and references, but it will serve a useful purpose in giving the psychical researcher of today some knowledge of Hodgson's personality and the value of his work.

H. DE G. SALTER

ERGEBNISSE OKKULTER FORSCHUNG. By Rudolf Tischner. Stuttgart, Deutsche Verlags-Anstalt, 1950. 212 pp. 4 plates. Dm. 14.50

It is a pleasure to welcome this new book from our old friend Dr Tischner and to see in it all his old knowledge and acuity. If there are some indications of insufficient acquaintance with the most recent literature, that is only to be expected. It is an additional pleasure to see this book as well printed and produced as ever.

Dr Tischner classifies the material under the headings of the unconscious, parapsychic phenomena, parapsycho-physiologic pheno-

mena, parapsychical phenomena, spiritualism, and haunting, with a final brief theoretical chapter, though there are theoretical reflections throughout the book. The author's general attitude may be described as a reserved acceptance of practically all the alleged phenomena of psychical research. Dr Tischner certainly goes further than the present reviewer would be prepared to accompany him. For instance, it is difficult to take Frau Silbert at all seriously, let alone devote several pages to her. On the other hand Dr Tischner makes only brief references to Rudi Schneider (I give the references, pp. 150, 152, as his name does not appear in the index).

These are minor blemishes on an excellent survey, which it is to be hoped will soon be translated.

Th. B.

SPIRITUALISM, REINCARNATION AND IMMORTALITY. By Marcus Knight, B.D. London, Duckworth, 1950. 128 pp. 6s.

This little book is intended for the more or less orthodox Christian who finds himself puzzled by the very inadequate teaching about eternal life which he often hears (or fails to hear) from the pulpit, as contrasted with the positive and widely advertised claims of Spiritualism. As such it ought to do a great deal of good, since Canon Knight has read widely and sympathetically and is very far from taking the simple-minded but unilluminating view that Spiritualism, being unorthodox, is ruled out of court. His readers will find themselves in possession not only of an outline of the main facts, but also of a real attempt to do justice to the background of motives which have made the Spiritualist interpretation so attractive to many, even while they have made scientists often so unwilling to consider the evidence at all. The importance of critical work such as that undertaken by the S.P.R. is fully recognized, with special emphasis upon the great difficulty of interpretation which is implicit in any exact analysis of the evidence. Canon Knight quotes Professor H. H. Price and Mr G. N. M. Tyrrell (whose name, curiously enough, he persistently mis-spells) to illustrate both the importance and the difficulty of the whole subject when it is scientifically treated. But he does not carry this part of his inquiry very far, since his main purpose is to show the inadequacy of the claim, often made by Spiritualists, that the evidence for survival has now been placed upon a factual basis, scientifically established. The more recent inquiries as to *psi*-phenomena and their significance for the re-consideration of the whole framework of the pattern within which science works are barely mentioned, and such problems as are presented by the

relation between telepathy and clairvoyance, contemporary, retro-active, and pre-cognitive, are not discussed at all. But Canon Knight had the ordinary Christian layman in mind, and not the expert in psychic research. Perhaps the greatest value of his book will be its encouragement to those who have been a little afraid of the whole subject and its full recognition of its importance when approached with a proper respect for evidence and for an adequate scientific and philosophical study of its meaning.

L. W. GRENSTED

JOURNAL OF THE AMERICAN SOCIETY FOR PSYCHICAL RESEARCH,  
Vol. 44, No. 3, July 1950. New York, A.S.P.R. \$1.50.

The American Society co-operated in the design of a questionnaire included in an article which appeared in *This Week Magazine*, entitled 'Are You Psychic?' Six of the questions were similar in type and one of them was: 'When something was lost, did you ever have a sudden feeling, "It's over there"—and find it?'. The shortcomings of this type of question are revealed if one poses another: 'When something was lost, did you ever have a sudden feeling, "It's over there"—and it wasn't?'. Most people would give an affirmative answer to both questions, and would probably admit, if pressed, that the second occurrence was more frequent.

Each of the six items, in fact, embodies two questions: (a) Do you, in specified circumstances, get a sudden impulse or impression? (b) If so, how often is it correct? Nevertheless, the questionnaire, as a preliminary survey, is probably adequate. Readers were also requested to do three simple E.S.P. tests with cards. Those answering 'Yes' to a certain proportion of the questions were invited to send in accounts of their most clear-cut psychic experiences, and the 920 replies have been classified.

The chief contribution is a paper by J. L. Woodruff and Mrs L. A. Dale on 'Subject and Experimenter Attitudes in Relation to E.S.P. Scoring'. After the test and before the subjects knew their scores, they were asked to fill in a questionnaire expressing their attitude to the experimenters and to the conditions. This carefully planned project yielded results which must have been rather exasperating for the experimenters. First of all there was no significant score total. Differential scoring according to attitude to experimenter was observed only with Woodruff and not with Mrs Dale; the difference was in the direction not expected on the basis of previous work. Subjects giving the experimenters a low rating for congeniality were the best E.S.P. performers and *vice versa*. The attitude of the



experimenters towards the subjects gave no clear-cut correlations with E.S.P.

E. P. Gibson reports a case of paranormal cognition with a trance medium named William H. Thatcher, which has some interesting features.

The last article is an unusually well-attested spontaneous case. Five persons of high integrity were discussing a case of super-normal clock-stopping (see A.S.P.R. *Journal* for July 1949).

Because we were skeptical we glanced at our watches and one member of the group mentioned how phenomenal it would be if our watches were to stop. Suddenly Mrs Margaret Wadsworth exclaimed that the watch she was wearing had stopped, and Mrs Cornelia Wadsworth noted that her watch had stopped at precisely the same moment. That was exactly ten minutes before eight o'clock. The hands of both watches remained motionless for exactly five minutes during which time they were examined by all five members of the group. The hands began moving again at five minutes before eight o'clock without external cause, and the two watches had been fully wound that morning.

The statement is signed by the five participants, and the article ends with some further discussion of the case.

D. P.

## CORRESPONDENCE

### TELEPATHY BETWEEN ANIMALS

SIR,—In Professor A. C. Hardy's interesting contribution to the *Journal* (May-June 1950), reference is made to possible telepathy between animals, with mention of Edmund Selous's book—*Thought Transference, or What? in Birds*. Those of our members who may be interested in this subject will find it treated at some length in William J. Long's book *How Animals Talk* (Harper, 1919). The author devotes two chapters to this question: 'Natural Telepathy', and 'Chumfo, the Super-Sense'. He gives many examples of this from his own experience, such as the *instantaneous* movement of a herd of hundreds of elk, when one of them has caught your scent. It is not a *ripple* of movement, such as one might expect to find were the result due to contagion; an elk a mile away is on the move the same instant as the one nearest to you. This, Mr Long thinks, is strongly indicative of telepathy.

Royal Dixon, in his books *The Human Side of Birds*, *The Human Side of Plants*, *The Human Side of Trees*, etc., is also of the opinion

that some extra-sensory faculty is operative, even here. Many have contended, as we know, that snakes, wasps, bees, spiders, and especially ants illustrate something closely akin to this. E. L. Bouvier, in his work *The Psychic Life of Insects*, says : ' Maeterlinck has spoken of the *esprit* of the hive. Is there, then, an *esprit* of communistic societies? And this *esprit*, is it not somewhat analogous, by the reaction of individuals upon individuals, to what Dr Gustave LeBon has called the spirit of the crowd? It is easier to indicate this problem than to solve it ' (pp. 348-9).

Some time before the last war, the Yugoslav Psychical Research Society was experimenting in this direction, and sent me some of its preliminary reports. I understand that Dr Rhine and his co-workers have also tried experiments along these lines. Many of us would be interested in knowing the upshot of these trials! Even the lowest forms of life are not above suspicion, in this connexion. Binet, in his *Psychic Life of Micro-Organisms*, writes : ' Chemistry lies at the basis of physiology, but chemistry does not explain physiology. . . . In these inferior beings, which represent the simplest forms of life, we find manifestations of an intelligence which greatly transcends the phenomena of cellular irritability '. Of insects, it has been said that, ' Isolated, the social individual is incapable of all useful endeavour ; in company with his fellows, it knows how to solve the greatest difficulties '. Is this not suggestive of some form of instinctive mental contagion? Perhaps one of our leading biologists or entomologists would supply us with a brief article upon this interesting question?

HEREWARD CARRINGTON

### THE LAWS OF EVIDENCE

SIR,—I gather from recent numbers of the *Journal* that Dr Thouless believes Frederick Marion to possess paranormal faculties. I have also heard Dr Thouless say publicly—at the Progressive League Conference on psychical research held in Brighton in February of this year—that he believes telepathy to be present in such B.B.C. programmes as the Piddingtons and ' Twenty Questions '. Now Marion has admitted to me that he achieves his results by normal means ; moreover, even I (I say ' even ' because I am very bad at parlour tricks) can duplicate such of Marion's results as I have myself witnessed. The Piddingtons have made no claim to paranormal powers, nor have the B.B.C. on their behalf. ' Twenty Questions ' is a drawing-room game, with varying personnel and rules, which anyone can play with similar results under similar conditions.

None of these considerations proves, to be sure, that telepathy

is not present in these performances ; but surely only because it is not possible to prove a negative. Dr Thouless's position is tantamount to saying that in his opinion telepathy is so common an occurrence that it must be preferred as an explanation to quick wit, skill in legerdemain, and the like. Does he believe that, or does he not accept the law of economy of hypothesis?

His attitude is endorsed by Dr Wiesner, who writes (p. 222), in support of Dr Thouless, that 'in the nature of things there could not be any evidence against the operation of *psi*'. Of course not, but is Dr Wiesner seriously proposing a reversal of the laws of evidence? Is he really claiming that the onus of proof rests on anyone who attributes a phenomenon to normal causes?

I ask these questions in no captious or controversial spirit : I am deeply concerned by these indications of a tendency not merely to abandon but even to treat with contempt the fundamental canons of scientific evidence.

THEODORE BESTERMAN

SIR,—Mr Besterman has a curious method of criticism. He attributes to me public utterances I have not made, elaborates from these an opinion I do not hold, and bases on this fabrication a charge that I exemplify a tendency to 'treat with contempt the fundamental canons of scientific evidence'.

I did not say at Brighton that I believed that the Piddingtons used telepathy in their B.B.C. performances. In reply to a question about the Piddingtons, I expressed the opposite opinion. I hope I added that, since I had little evidence on the matter, my opinion about it was not worth much. Also, I did not say that I believed that telepathy entered into the game of 'Twenty Questions'. I have heard this suggested and it seems possible, but I have done and I know of no experimental work on the matter. I may have said in the discussion at Brighton that it was possible ; certainly not that I believed it was so. On the other hand, it is true that I believe that Marion has paranormal capacities. This belief is not based on Marion's public performances (which I have never seen) but on his success in experiments carried out by Dr Wiesner and myself designed to eliminate other possibilities of explanation.

The fundamental principle of science that I recognize is that if one wants to know what is the truth on a particular problem, one must find out, not by applying high-sounding laws in one's arm-chair, but by designing and carrying out an experiment. Failing that, one must reserve judgment or form a tentative opinion from such evidence as is available while remaining ready



to change that opinion when better evidence is available. For that fundamental principle, I venture to think that I show more respect than does Mr Besterman.

ROBERT H. THOULESS

SIR,—Mr Besterman says by way of a first comment that I am right. Then he implies that I am claiming that explanations by paranormal causes should have some kind of precedence over explanations by normal causes.

I have never suggested this and do not feel called upon to defend a proposition which I have not made ; even less one which even Mr Besterman accepts as true.

B. P. WIESNER

### ‘AN ADVENTURE’

SIR,—The reviewer of the *Journal* of the American S.P.R. for April 1950<sup>1</sup> reproaches me for not having held back my article, ‘Is there a Case for Retrocognition?’ for revision, in view of the fact that W. H. Salter’s review of the evidence for *An Adventure* had appeared in your *Journal* for January–February 1950.

But I had not seen Mr Salter’s article when, towards the end of February, the final proof of my article (which had been sent in MS to the American office on July 6th, 1949) was returned to the printers. Some time earlier I had had an intimation that Mr Salter’s article had appeared, and it was thought appropriate to add a footnote which would have the effect of directing readers to the January article. I am quite sure that there was no reasonable possibility of withholding my rather long article at that stage, or of revising it to do justice to Mr Salter’s arguments. The correct view is to regard the two articles as having come out simultaneously.

I must also ask leave to protest against *An Adventure* being termed ‘the *pièce de résistance*’ of my article, which conveys the impression that I am an advocate of its claims. I summed up my views in that connection by saying, ‘It is hard to see any indication that other than purely subjective creations of the mind are involved, images built up not only from unconscious knowledge acquired since infancy in environments impregnated with historical associations, but also from extra-normal awareness of the sources of additional knowledge.’

<sup>1</sup> See *Jnl S.P.R.*, July–August 1950, p. 288.

Your reviewer might have added that while I unfortunately did not have the advantage of seeing Mr Salter's valuable discussion, it is also the case that I have been able to point out aspects of the matter not dealt with by Mr Salter, notably the circumstance that Miss Moberly saw visions of a comparable kind on other occasions, whatever their cause may have been. The statement to that effect by Miss Edith Olivier in *Four Victorian Ladies of Wiltshire* (1945), supported by members of the Moberly family, cannot be ignored, and is one reason why the case is not 'firmly wedged into its correct pigeon hole'. And I have been able to indicate several hitherto unmentioned sources of the contents of Miss Moberly's mind, such as Flammarion's *Lumen* (1897), and the Baedeker's *Paris and Environs* (1900). Here I may add yet another—A. Brossard's *La France du Nord* (1900) whose crude coloured photographs, especially that on p. 121 ('Petit Trianon—Le Hameau') correspond to the still tapestry-like scenes described in *An Adventure*.

Further investigation of the *An Adventure* case is not to condone the Moberly-Jourdain interpretation. Grossly ignorant of organized psychical research, they went sadly adrift, but many others who have had apparently extra-normal experiences have been prejudiced and naïve assessors of them.

W. H. W. SABINE

Our reviewer writes: 'I mistakenly interpreted the words in the footnote "ready to go to press" as meaning that the article was not yet in proof. I am very pleased to withdraw the statement referred to by Mr Sabine, with apologies to him and to the Editor of the *American Journal*.'

## OBITUARY

MRS LEONORA PIPER

WE greatly regret to record the death of Mrs Leonora Piper, whose name will be familiar to every reader of this *Journal*. It is intended to publish a fuller tribute to her memory in our next issue.





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